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PSYCHOLOGY AND SCIENTIFIC METHODS

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THE JOURNAL OF PHILOSOPHY
PSYCHOLOGY AND SCIENTIFIC METHODS

SCOPE, METHOD, AND PSYCHOLOGY IN ECONOMICS[1]

IT is probably true that the last word in economics will be said only after the last work has been done in philosophy or in psychology or in both. Equally clearly, however, must a finished philosophy or psychology await the completion and perfection not only of economic science, but of pretty much all science at large. Another thing, however, is it to say that any science must postpone its beginnings until some other or all others shall have achieved perfection. It seems indeed certain that, if there can be no political economy till all the issues of psychology are settled, there can never be any political economy at all.

But it is none the less unfortunate that the economists themselves give no promise of arriving at even a factual and descriptive statement of what their central problem or concept objectively is. No matter how modest as aspiration or simple as task the thing on the face of it may appear to be, it yet is indefinitely far withdrawn from immediate prospect. Not often, in truth, does any economist succeed in so far arriving at peace with himself as to avoid confusions of the relational with the non-relational meanings of *value*. Instead, his usage is prone to range through all the various steps and varieties of meaning—from utility, through marginal utility, subjective appraisal and comparative appraisal—and reaches its farther terminal in the objective fact of exchange relations as expressed in the market and made concrete in price. It does not then lie with any economist to complain that in other fields of thought in which the same term is domesticated the same confusions appear. To economics can belong neither the exclusive use of the term nor the exclusive exploitation of its confusions. It is, however, mainly at the charge of the economists that ethical values and

[1] Henry Waldgrave Stuart, "Phases of the Economic Interest" in *Creative Intelligence*, pp. 283–313. New York: Henry Holt and Co., 1917. Warner Fite, "Moral Valuations and Economic Laws," in this JOURNAL, Vol. XIV., p. 5. J. Viner, "Some Problems of Logical Method in Political Economy," in *Journal of Political Economy*, Vol. XXV., No. 3, March, 1917.

esthetic values and nutritive values are constantly asserted to be at one in ultimate principle with economic values. Clear thinking requires that merit be held clearly distinct from comparative merit, that things be recognized not only as beautiful, but as beautiful in varying degrees, that foods be thought of not solely as all nourishing, but as more or less nourishing relatively to one another. If, then, it be the economist's problem to explain the objective *relations* of exchange between things, and if these relations receive expression as economic *values*, and if also a technical term should mean always not more than one thing and always the same thing, it should follow that only in the sense of desire or approval or hunger, utility or worth or appraisal, or emphasis or significance, can the value phenomenon offer common ground between economics on the one side and ethics or esthetics or dietetics on the other. These other sciences may, for their purposes, spread the term *value* over any or all of their non-comparative data, or may even, if it so please them, remain in perpetual flux between notions of merit and of relative merit, between ugliness separately considered and ugliness regarded comparatively, or between digestibility viewed as an absolute chemical fact and viewed as merely the expression of the relative ease of digestibility of different foods. But if value is to mean to the economist merely relations of exchangeability, it should mean solely and always this one thing, connoting always the idea of rate or ratio; or, if this be overmuch to ask of frail human thought, the term *price* should be substituted for *value* wherever the objective facts of the relations of goods in the market are to be expressed. This would mean the abandonment of the term *value* to the uses of the other sciences to make whatever best they should be able out of its various vaguenesses.

Little, however, of this protest or aspiration or admonition can rightly refer to Professor H. W. Stuart's admirable and suggestive "Phases of the Economic Interest." While his essential problem is formulated as the process of *valuation*, value is to be understood as connoting with him what the economists have in recent years been fairly well agreed in calling *utility*. His purpose is to render an adequate account of the psychology of desire: Why do we want particular things? On what basis do goods attract us in such fashion that they come to be desired by us as objects between which selection must take place and choice be declared?

Note that the problem is, therefore, not precisely the psychology of *choice*, the way in which we relate things to one another in the making of our purchases, but rather the way in which they come to be objects of desire for us in such sort that we may relate them and select or choose among them. It is thus that, in Professor Stuart's

view, the field of economics widens, in this aspect of it, to include not merely desires for food, clothing, and shelter, but all aspects of esthetic and ethical appreciation. It follows that he allows short shrift to all varieties of hedonism as purporting to offer an account of the actual process of desiring. Doubtless one may desire those things on the control of which in his experience certain advantages have been found to depend, though even here, perhaps, the fact that the advantage achieved—that he turned out to like what he got—leaves the attendant satisfaction not so much explaining the desire as the existence of the desire explaining the satisfaction. But in any case, the quest of pleasure or even of gratification is no adequate account of human desires and strivings. Not only is much of our desiring instinctive or habitual or impulsive rather than reflective or calculative, but these desires are themselves undergoing development; each one of us, therefore, is always in the process of becoming, for the purposes under examination, a new man, the desire itself undergoing modification by the very fact that during the process in which we are relating desires and are thus arriving at selection and choice between them, the opposing terms of the choice achieve new definition and new meanings.

It is, therefore, the process of "constructive comparison" to which Professor Stuart calls especial attention. The choice between alternative purchasable things may involve—in some measure, I suppose, always does involve—"alternatively possible systems of life and action. . . . Each system may be criticized and interpreted from the standpoint of the other, each may be supplemented by suggestion, by dictate of tradition, and by impulsive prompting, by inference and by conjecture. Finally in experimental fashion an election must be made" (p. 348)—with possible further modifications of the desires involved and of their relations to one another.

And more than this: among our different desires—the different interests by virtue of which we act and in the furtherance of which we make these constructive comparisons leading to choice—must be included our desire to become men of new levels of desire and of interest; else why try to learn to like olives, or spend so great efforts to become interested in music or in art, or to form new acquaintances or friends? Facts of this sort illustrate the larger generalization that human beings desire *novelty*, something of which past experience can not in any ordinary sense have discovered the advantage or fixed the desire—desires for something that, never having known, we can not have enjoyed—the new, the untried, the unexpected. "But the *impasse* is conceptual only;" we do know the new things to the extent of knowing them in this aspect of their newness. In any other aspect they are indeed unknown. But still we know that,

among the things that have been known only in this aspect of newness and strangeness, many have later achieved significance for us—the more, perhaps, that they were new and strange. Thus novelty comes to be desired in its own right. The aspiration to be different men, to achieve a new plane of interests or of ethical or esthetic standards, is merely one expression of the fact that we are human beings in an unfolding process, and that the inexperienced falls within our range of desires.

Nothing of this need arouse protest. For my own part I accept it all, insisting solely that clarity of economic analysis becomes impossible if we have to regard as *values* the items between which choice must be declared—if, for example, it is to be asserted that "we have not chosen the new because its value, as set beforehand, measured more than the value of the old, . . . the old . . . worth less" (p. 348); and emphasizing also what seems to me the obvious truth that "there are apparently no valid objections to this view to be drawn from the logical type of marginal utility analysis" (p. 349). Nor, under similar reservations, need there be dissent from Professor Stuart's next and most important position: "The issue finally at stake in any economic problem of constructive comparison is an ethical issue. . . . A price agreed to and paid makes a real transition to another level." From our dead selves we move to higher—or lower—things. We do not desire anything in separateness from other things, but to go along with them; therefore every choice is measurably a choice between competing systems of living—therefore is ethical in the non-relational sense, a "valuation" of each separate system, the two together providing a basis for that relative valuation fundamental to a declaration of choice between the competing items or systems. There are, then, no choices so trivial as to fail of an ethical aspect, if only we come into the consciousness of all the relations, the reactions, and the implications. Nothing is neutral to him who lives always in the presence and service of God, or is always seriously intent on the direction and meaning of his own development.

Precisely, therefore, because there is no man acting always in economic affairs in strict conformity with egoistic or selfish or hedonistic principles, and because human desires, reaching out toward objective things, contain always or often these admixtures of adventurous or ethical interest as well as of impulse and instinct, the economic analysis which fails to allow for the complexity and variety in human motivation becomes either a gross caricature or a futile abstraction. *Utility,* as the mere expression of desiredness, requires still a new psychology behind it to explain it. The ethical is not to be regarded as distinct from the economic or as antithetical to it, but as inseparably part and parcel of it.

Thus artistic and religious and ethical interests fall within the economic field, else paintings, altar candles, and charity hospitals would have to fall out. A psychology for human beings as they are and as they actually desire and choose is necessary to an economic science which shall report things as they actually take place.

All the foregoing would readily command general acceptance—my own included—if only the authority of another brilliantly capable philosopher and psychologist, Professor Warner Fite, were not—at least in seeming—directly opposed. To Professor Fite whatever is ethical is not economic, or, more precisely, in whatever degree it is ethical in that degree it is not economic. That the phenomena of actual life range at all points between these two opposing extremes or poles amounts almost to saying that there are nowhere any purely economic phenomena, nowhere any facts which, tried by descriptive and concrete tests, the economist can be trusted fully and accurately to report. And all this must be true precisely because men are never, in their economic activities, entirely selfish or ruthless without reserves or limits. In Professor Fite's view, economics is only so far a science—and only dubiously a science at that—as it assumes the consistent selfishness of all economic activity, as it abstracts from all other human interests and motives, not denying their existence, but unable to make anything of them. To that sort of business which is pure business must the business of economics be restricted. So far as any business is something more or less than business at 100 per cent. of purity, it is refractory to economic tests, gets away, escapes, or, if caught, is caught merely to be misinterpreted and misreported.

Thus, while Professor Stuart asserts that all these questionable things are within the economic field, Professor Fite is able to find no economic field excepting on the terms of leaving these disturbing things out. Actually, of course, they are always present. The question for the economist is, therefore, whether to make room for them by seeing them or by not seeing them. I take Professor Fite to hold that only so far as they may be left out—and thus, in truth, not far—can there be any science of economics.

If only the philosophers and psychologists could agree! And so long as they can not or will not, what is there for the economist safely to do? For my own part, I am content to agree with both—in the sense that I prefer a view which dodges these puzzling and complicated issues, committing myself to neither. If this be a running with the hare and a hunting with the hounds I am content, finding nothing else to do short of this deliberate much dodging and brilliantly multiple running. But if nothing can be done with eco-

nomics till all the relevant psychological issues are settled—till the last philosophic word has been said and has been accepted—there must be still 10,000 more years to wait after a first 10,000 years are done. Why, once again, should not psychology wait till we get everything completely and amicably settled in economics? Or physics stand hat off in waiting on chemistry? Or the other way about? Or biology on both? For myself it seems sufficient for immediate purposes—and safe withal, since all the warring schools so far agree—to hold merely that men do desire and do choose. I find it unwise to accept hedonism, but unnecessary to reject it. I am content to forego certain attractive fields of analysis until the scouting squads of these other sciences have made them safe for untechnical wayfarers—the thickets cleared, and all ambushing patrols accounted for. I know it would be far better otherwise—but so it is. Economics, it is true, will never achieve perfection on this basis; but it will never be at all on any other—unless, indeed, there shall be somewhere an economist so near approaching omniscience as to displace within their special fields the authority of the investigators appropriate to them. But a more modest caution would appear to be the more fitting rôle. It is better to swallow no medicine so long as it is a chance if it be not poison. Experience does not always recommend the unknown. If one can manifest no slightest taint of hedonism unless thereby all of his economic reasonings must be declared infect, how shall any one, short of imperative need, dare to take risks with anything either new or old? The fact that any particular view holds the field to-day is the best guaranty that it will have lost it a generation hence. The only view that one could safely tie up with must be some view that is not yet and has never been—something hard to get satisfactory hold of—if, indeed, in any wise it could be discovered. Accepting, therefore, the hazards of the notion that men have desires and of interpreting *utility* to mean nothing more than this mere fact of desiredness by an individual, I find it unnecessary to assume further that all the various influences and all the different processes that lie back of this fact of desire, and that lie back also of all the different desires of different men, are adequately and exhaustively known to me or to any brother economist. Stuart may be right—I suppose that he is—in large part, and as far as he goes, and as subject to later troubles with his brothers of the psychological craft. But I decline to trust these bridges until the structure is better tested, till no man will promptly deny everything else that I think because I do also think some one thing that Professor Stuart thinks. And how can he be sure that he has accounted for all of the things that people, or some people, are interested in, and by which their ways of expenditure are affected? Professor Love-

joy is sure that we are interested in our posthumous selves—not in our destinies in the heaven or the hell whereto we are moving, but in our earthly selves and in our standings and reputations here, where we are not going to be. For do we not aspire to future fame or to kindly recollection? Foolish people we are, some of us, perhaps all of us. Some people, again, are reported to be active in keeping their dead ancestors' ghosts well fed—thus affecting the market demand for current comestibles.

I can assent also to Professor Fite's view that economic laws can have the degree of validity and certainty of laws of the physical sciences only on the assumption that economic phenomena are uncomplicated by choice and agreement. *In this sense of economic law,* "no situation which implies the participation of living human beings can be dominated exclusively by economic law. . . . So far as they move consciously, they are moral agents, so far as they move unconsciously, they are physical facts like any other physical fact; and it is the bare physical fact that lies at the basis of economic law. . . . The opposite of natural law is personal choice or agreement. . . . The economic process is a function not of the world of social consciousness, but of the larger world of social unconsciousness" (pp. 16–17). Only, therefore, when men act, and in the degree that they act, as if they were blind, non-willing, and unmoral items of opaque fact—not necessarily in the strict sense of the word unconscious, but unguided or at least unswerved by the fact of consciousness—is there place or possibility of economic law in Professor Fite's interpretation of the term. But still it is possible that the phenomena of suicides or murders or drunkenness can be generalized and that reasonably accurate forecasts of them may be made without the assumption that the phenomena are, therefore, vacant of consciousness or choice.

But I take it as a fair interpretation of Professor Fite's position to regard such human conduct as conforms to the hedonistic hypothesis, if any such there actually is, or such conduct as is completely selfish and ruthless in its pursuit of gain, as sufficiently approximating the automatic character of an entirely competitive "unconsciousness" as to render this conduct amenable to the methods and adequate to the requirements of economic law.

It will be obvious that our two philosophers need not, and very possibly do not, at all disagree as to the psychology of desire, but only as to the delimitation of the economic field, or possibly also as to the meaning of economic law. In substance it is to the view of Professor Fite that economists will have to turn for whatever comfort there is in the situation. It is precisely this fact that

in the social field we are dealing with the things that we best understand and most intimately know—the field of consciousness and of ethical appraisal and of choice—that makes the subject-matter especially inappropriate to the methods of quantitatively exact scientific law. To some of us it has even appeared that precisely here is the great and significant advantage attaching to what the laboratory science men dub the quasi-sciences as compared with their own so-called exactitudes; we do a little know and understand what we are talking about—men, their motivation, their needs, and their choosings. Residing inside ourselves, with whom we live in partial acquaintance, we are able to inspect the process from the inside. But these men of the physical sciences are always in presence of things that are essentially mysterious. The more cautious of these investigators have come to guard themselves from talking of forces, affinities, attractions, repulsions, energies, causes, or results. They simply report what different things occur and in what simultaneities or sequences—a purely exterior and descriptive statement of objective fact. In a sense, therefore, as Professor Fite insists, there is in all this for the economist both advantage and handicap. We shall never be "scientific," for that very best of reasons that we have no need to be: "Scientific method is irrelevant to the familiar and concrete; as a rule, we reserve it for the more remote and inscrutable. Scientific knowledge is knowledge by simplification; in other words, it stands for relative ignorance . . . For the concrete qualities of a substance the chemist substitutes an atomic structure; for varieties of color the physicist substitutes ratios of vibration of ether; for the relation between color qualities as such the physicist can tell you nothing," and it is to be added that his ether and his atoms are themselves unknowns, mere working guesses or imaginings.

It must result from Professor Fite's position that, while we may generalize even the phenomena in the field of human consciousness and choice, we can not force our phenomena into such categories of law as shall assimilate economics to the methods and laws of the physical sciences. It must be admitted that, if the notion of causation is in place at all in the field of human activities, it must be in the sense of stimulation and response rather than of some deduction from the notion of the conservation of energy. If causation holds in both fields, it holds in different senses and under different interpretations.

But Fite and Stuart are in entire agreement that the most effective method of studying social activities must approach the problems through an examination of the way in which human desires and choices actually proceed—the psychological approach, the method of

study from the interior view rather than by the uncomprehending devices of exterior observation. Doubtless the two methods, the points of view both of the agent and of the observer, as Professor Fite has defined them, are essential. The exterior view may indeed greatly contribute, as witness the services of statistics. And still it must be the interior, the agent, view that furnishes the key for the interpretation of the statistical materials.

But there are other methods aside from the suggested appeal to a purely behavioristic psychology—if psychology of any sort the consistently exterior study of any field of phenomena can possibly be regarded. In truth, it is not essential to economic generalization that any one influence or motivation as over against any other influence, or all influences together as over against any several or any one, be accepted as the organizing principle on which the facts shall be explained or presented. Avoiding the assumption that men are always selfish, that in their economic activities they always act unmorally or calculatingly or consistently or rationally, and avoiding also any methodological abstraction from other influences admitted to be actually or possibly present, we may proceed effectively and in relative security, if only we take it to be true that every man desires and chooses—so far as he sees—to get the greatest possible differential between sacrifice and reward, the most for the least. This is not much more than to assert that, whatever the motives and purposes, each man follows what is for him the line of least resistance or of the strongest pull. "We are concerned with his ends and not with his motives."[2]

But still we are making the same ultimate assumptions, that men (1) have desires and (2) make choices between those satisfactions that are mutually exclusive, and between the means that are open for the achieving of the particular satisfactions. It seems, indeed, that according to any method of the interior view, the thing could not otherwise be conceived to go on. These two assumptions recommend themselves as presumably safe by the fact that they are nowhere seriously contested.

But still I wonder and am afraid. Why has no one put in issue the fact of choice? or made trouble for us economists about the psychological process of it? After all, are there any choices at all? I recall that it did once appear to me that there are none, in any important sense, and that "even to say that we choose in the direction of the stronger desire is tautology, since choice is merely the fact of the stronger desire."[3] But I regret this statement as an unfortu-

[2] Viner, *Journal of Political Economy*, p. 248.

[3] From my review of Fetter's *Economic Principles, Journal of Political Economy*, Vol. XXIV., No. 4, p. 320.

nate and gratuitous excursion. It seems to me now that choice is something more than the mere emergence and victory of the stronger desire. I am sincerely sorry that I adventured at all into the psychological quagmire. And still the fact is itself important to the present purpose as showing that there is a mess, a problem, an issue, or may some time be one. But if ever there shall be one, and still the psychologists will leave it open to us, we can retire to a position of naïve acceptance of the mere fact of desire. *Utility*, in this sense, will still remain to us. And *marginal utility* is a logical as well as an actual derivative. Dispossessed of choice, we can not accurately have it open to us to compare marginal desirednesses or to set them over against each other for relative appraisal. Instead, they will have to set themselves over; and out of this process of the mutual comparison of themselves, the weaker desire will come to be overborne by the stronger; and to the satisfying of this stronger desire the purchase money in hand will direct itself as outlay; and all again will be as well as may be.

H. J. DAVENPORT.

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THE RELATION OF TRUTH TO TESTS

WITH the exception of the pragmatists, modern philosophers are coming to recognize the necessity for a clear distinction between the definition and the criterion of truth. But too often the recognition is only nominal, and a writer who will use a page or two in explaining the distinction will violate it in the same chapter. There is nothing, I believe, which would aid more greatly in the solution of the truth problem, than an explicit agreement, consistently held to, that what may be said about a criterion of truth may have no influence at all upon its definition, and that a statement regarding the definition or meaning of truth may be entirely out of relation to the criteria by which truth is to be known.

Since the practical aspect of the truth question is to be able to distinguish the true from the false in situations that actually confront us, the pragmatists have naturally fastened their whole attention upon the criterion of truth, and have formulated the definition in terms of the criterion. Thus Professor Dewey says: "Truth is a character which belongs to a meaning so far as tested through action that carries it to successful completion. In this case, to make an idea true is to modify and transform it until it reaches this successful outcome: until it initiates a mode of response which in its issue realizes a claim to be the method of harmonizing the discrepancies

in the given situation. . . . Verification and truth are two names for the same thing." To illustrate: "I hear a noise in the street. It suggests as its meaning a street-car. To test this idea I go to the window and through listening and looking intently—the listening and looking being modes of behavior—organize into a single situation elements of existence and meaning which were previously disconnected. In this way an idea is made true."[1]

Now this identification of truth with verification, or of the definition with one of the tests for truth, leads one to such questions as the following. First, how many tests are necessary before the idea becomes true? My first test may well be inadequate—later tests may show it to have failed entirely to prove what I thought it proved; but if the first test satisfied me, if it "released the tension" in my experience and "restored harmony" therein, did it make my idea temporarily, even momentarily, true? Secondly, by whom are the tests to be made? We would agree that we can not identify truth with the conclusions reached through tests made by a lunatic or an imbecile, or by a normal person suffering an hallucination, or in some cases by an experimenter not an expert in the field under investigation. Surely the mere feeling of confirmed expectation, in just any one who chooses to test an hypothesis, is not sufficient to "make" a truth. Suppose that a man walking in a garden, to take another of Professor Dewey's illustrations,[2] detects a certain odor, forms the hypothesis that it belongs to a rose, searches, sees the rose, and feels his expectation fulfilled—he should, according to the theory, make his hypothesis true as soon as he is completely satisfied of the fulfilment of his expectation. Yet suppose that as the walk is continued the thing thought to be a rose is found to be only a spot of red light thrown at this point upon the foliage through colored glass. It is certainly a strain upon language to say that an hypothesis confirmed by an illusion is a truth. And as to the tested beliefs of the inexpert, their inadequacy is but too familiar: an amateur in chemistry, for example, may form, and confirm to his entire satisfaction, the hypothesis that the presence of a certain metal is indicated by a certain color of flame, whereas, in spite of some coincidences in these elementary tests, this metal and this flame-color may have nothing whatever to do with each other.

The frequency of *mistakes* in verification would seem to show that experimental results can not be completely identified with truth, as they must be when truth is defined in terms of verification.[3]

[1] *Influence of Darwin upon Philosophy and other Essays*, pp. 139–141.

[2] From "The Experimental Theory of Knowledge," *Mind*, N. S., 15, July, 1906.

[3] An interesting case of error confirmed as truth by the experimental method in the hands of scientists is given by Venn in his *Empirical Logic*, p. 510.

While we may agree that the experimental method is perhaps responsible for the greater part of modern science, and acknowledge it to be the best test of truth now in our possession, yet since even experimental verification is by no means an infallible method we can hardly go so far as to construe it as portraying the very meaning of truth. Unless a test is an infallible test, it can not be used as giving a definition, for that definition would not be reversible. Such a treatment of the truth problem gives as a definition: "True beliefs are those which have been experimentally verified" which, when converted simply becomes: "Beliefs which have been experimentally verified are true." If, as I have just tried to show, the latter assertion does not always represent the facts, then the former proposition fails as a definition.[4]

It is, of course, possible to reply that this depends on what we mean by "being true," and that both these propositions are true in case the first is once admitted as a definition. But this way out involves too many absurdities to be feasible. That ghosts walk in the night would then be proved true by the first white object which confirmed an expectation; that the presence of potassium is indicated by a blue flame might then be a true idea in the mind of a freshman even though every existing text-book in chemistry denied it; and so on for numberless cases. To make experimental verifications always true by defining truth itself simply as that which results from experimental verification is indeed to insure their coincidence, but to insure it by strong-arm methods hardly legitimate. In such a way might one make beauty coincide with art, or moral excellence with patriotism.

Another objection to defining truth as verification is that this procedure apparently fails to provide for accidental discoveries. The beliefs so forced upon us seem to be true in advance of verification. To take an instance: If while motoring we suddenly round a turn in the road and see a lake spread before us, our belief from that time forth that there is a lake at this point is a true belief, quite apart from experimental verification. To take the experimental theory of truth literally would oblige us to say that every accidental discovery must be rediscovered before it is true—that we must solemnly set about leading up to this experience again through a succession of prediction and verification in a make-believe experiment.

That Professor Dewey does not himself consistently adhere to this identification of truth with the results of experiment is apparently shown by the fact that in all his writings in defense of the

[4] "The test of a definition is the exact equivalence of the sphere of its subject and predicate."—Article on "Definition" by Adamson in Baldwin's *Dictionary of Philosophy and Psychology.*

theory he has never asked us to make an experimental verification of it. To be consistent, we would have to conclude that the theory, therefore, has never become true.

As a last criticism we may point out that although the definition of truth in terms of experiment is an attempt to enrich philosophy by material drawn from science, the analogy between the two disciplines is not strictly conceived. For, if the scientist in conducting a series of tests finds some results which seem, we will say, to show the presence of a certain element, and others which do not indicate its presence, then his conclusion is not that the element is present a part of the time and absent a part, but rather that he is not sure about its presence; and similarly, if certain tests of the truth of an hypothesis bring fulfilled expectations and others do not, then the conclusion ought not to be that the hypothesis was true for a time, false for a time, and then true again (though this is what we are asked to believe if we *define* truth in terms of fulfilled expectations), but rather that we are not sure about its truth.

Mr. Bertrand Russell makes the necessity for a distinction between the meaning and the criterion of truth transparently clear. He says: "If you wish to know whether a certain book is in the library, you consult the catalogue: books mentioned in the catalogue are presumably in the library, books not mentioned in the catalogue are presumably not in the library. Thus the catalogue affords a *criterion* of whether a book is in the library or not. But even supposing the catalogue perfect, it is obvious that when you say the book is in the library you do not *mean* that it is mentioned in the catalogue. You mean that the actual book is to be found somewhere on the shelves."[5]

Applying this distinction to the truth problem, Mr. Russell finds the meaning of truth to be correspondence with fact, and its principal criteria to be self-evidence and coherence. But the discussion here, unfortunately, is much less convincing. He reaches this conception of the meaning of truth by the over-simplified process of examining coherence and finding it inadequate as a definition, and from this concluding, "hence we are driven back to correspondence with fact as constituting the nature of truth."[6] The possibility of still a third theory of truth seems almost too obvious to be overlooked. As to the criterion of truth, Mr. Russell's treatment is limited by the fact that it is acceptable only to those who can embrace the realistic metaphysics, since on this criterion all that we know goes back at some point for its proof to a direct and immediate per-

[5] *Philosophical Essays*, p. 137.
[6] *Problems of Philosophy*, p. 193.

ception of reality as it is.[7] And even the immediate acquaintance with reality does not constitute an ultimate criterion of truth, because, as Mr. Russell says,[8] we can not know in any given case whether we are genuinely or only apparently in touch with reality. Since, then, Mr. Russell's definition of truth seems to claim credence principally through showing that an alternative definition is still less tenable, and since his ultimate test of truth is hardly one that is either widely acceptable or practically workable, it seems necessary to conclude that one must look beyond this type of realism, as well as beyond pragmatism, for a satisfactory and satisfactorily defended theory of truth.

It is well worth one's while to examine the solution offered by the man who founded pragmatism and then refused to abide by its later developments. C. S. Peirce, the pragmaticist, relies upon experiment as his principal test for truth, but formulates a definition independent of any particular test. "Truth is that concordance of an abstract statement with the ideal limit towards which endless investigation would tend to bring scientific belief, which concordance the abstract statement may possess by virtue of the confession of its inaccuracy and one-sidedness, and this confession is an essential ingredient of truth."[9] "The opinion which is fated to be ultimately agreed to by all who investigate is what we mean by the truth."[10] This ultimate agreement may be presupposed, for "all the followers of science are fully persuaded that the processes of investigation, if only pushed far enough, will give one certain solution to every question to which they can be applied."[11]

To define truth in terms of ultimate opinion is of course to define it "psychologically." But the word *ultimate* rules out most of the objectionable aspects of a psychological definition—it avoids the vacillating, the personal, the prejudiced, and so on, in opinion. This definition is free from ontological entanglements. It is not necessary, with this meaning of truth, to presuppose any one theory of the nature of reality or of the manner of our getting into relation with it. This, it seems to me, is its particular advantage as compared to the correspondence theory of truth.

This statement of the meaning of truth is obviously useless as a criterion of truth. Not only are we aware that we have no means of knowing at present what will be ultimately accepted, but we realize

[7] *Ibid.*, pp. 75, 207.

[8] *Ibid.*, p. 214.

[9] Article on "Truth" in Baldwin's *Dictionary of Philosophy and Psychology.*

[10] "Illustrations of the Logic of Science," in *Popular Science Monthly*, 1878, Vol. XII., pp. 299, 300.

[11] *Ibid.*

also that contemporary acceptance is always a poor indication of ultimate acceptance. It is necessary then to look elsewhere for a criterion, and this Peirce finds in scientific experiment. "Truth . . . is distinguished from falsehood simply by this, that if acted on it will carry us to the point we aimed at and not astray."[12] "When we speak of truth and falsity, we refer to the possibility of a proposition being refuted; and this refutation (roughly speaking) takes place in but one way, namely, an interpretant[13] of the proposition would, if believed, produce the expectation of a certain description of percept on a certain occasion. The occasion arrives: the percept forced upon us is different. This constitutes the falsity of every proposition of which the disappointing prediction was the interpretant."[14] "Verification will not consist in searching the facts in order to find features that accord or disagree with the hypothesis. This is to no purpose whatsoever. The verification . . . must consist in basing upon the hypothesis predictions as to the results of experiments, especially those of such predictions as appear to be otherwise least likely to be true, and in instituting experiments in order to ascertain whether they will be true or not."[15] As a scientist who has spent most of his earlier life in a laboratory, Peirce proposes scientific experiment as the criterion of truth. In this he is at one with the more conservative of the later pragmatists.

This seems to me an overworking of a tendency in itself excellent. It appears obvious that there is no absolute necessity for making experiment the *only* test of truth. Its claim to an absolute equivalence with the truth-criteria is hardly better founded than its claim to being equivalent to the meaning of truth. This can not but remind one of present-day claims of certain nations of the earth that are said not to be content with a share in the political interests of the world, but insist upon nothing less than complete domination of the world's affairs. So might experiment appear, in the hands of its friends, to wish to dominate the whole field of truth-criteria.

It ought to be possible, however, after concluding the search for a satisfactory definition of truth, to allow a place to a number of different tests, all to a certain extent effective in securing truth as thus defined. Experiment might be allowed first place, as the most decisive test of ancient beliefs and the most fruitful method of discovering new truths. Where it can not be applied, we may utilize,

[12] *Ibid.*, p. 14.

[13] "Any necessary inference from a proposition is an interpretant of it."

[14] Article on "Truth" in Baldwin's *Dictionary of Philosophy and Psychology.*

[15] Article on "Verification" in Baldwin's *Dictionary of Philosophy and Psychology.*

in the next place, coherence.[16] Under certain circumstances we may test by the clearness and distinctness of the idea, by simplicity, absence of exceptions, inconceivability of the negation, and so on. We may to some extent test our tests, and thus be able at least to reject those which are obviously ineffective in promoting truth in the sense of permanent agreement after investigation. On these grounds we must, according to Peirce, reject as truth-tests, *e. g.*, "tenacity" or the exercise of the will to believe, authority, and agreeableness to reason.

The result secured by any test or tests must be considered to be, however, only a higher or lower degree of probability. The formal criteria, it is true, give us within their sphere absolute certainty, provided the premises from which the reasoning starts are true. But since the truth of these premises can not be proved by the formal tests, and since no other tests are infallible, it seems best to call the results secured by the formal tests not absolutely true, but absolutely valid, this distinction allowing us to ascribe all deserved potency to the so-called truth tests (really validity-tests) of formal logic, while at the same time considering them to result only in probability.

Propositions fail, indeed, to be exactly true both because of the imperfection of our tests and because of our inability, when we have secured a proof, to state our result precisely. Such a method of looking at the truth problem is, I believe, that of science, and puts us directly in touch with the conclusions reached by scientists. Thus Poincaré says: "No particular law will ever be more than approximate and probable. Scientists have never failed to recognize this truth; only they believe, right or wrong, that every law may be replaced by another clearer and more probable, that this new law will itself be only provisional, but that the same movement can continue indefinitely, so that science in progressing will possess laws more and more probable, that the approximation will end by differing as little as you choose from exactitude and the probability from certitude."[17]

Our progress toward truth as ultimate agreement will apparently lie in the double process of rendering our statements more precise

[16] However, this test will not, in the opinion of the pragmatists, procure us real knowledge or truth. Thus Professor Dewey says in 1917: "Thinking gets no farther, as *thinking*, than a statement of elements constituting the difficulty in hand and a statement—a propounding, a proposition—of a method for solving them" (*Essays in Experimental Logic*, p. 13). This position is necessary, of course, only in case one has accepted Professor Dewey's definition of truth, from which it follows immediately.

[17] *Value of Science*, p. 130. A similar position is taken by A. Sidgwick in *Mind*, 1914, pp. 99–101.

and of perfecting the tests for their truth. The latter may involve a better working out of the technique of the use of some of the known tests, or the discovery of new tests (as of experimental verification in modern times), or a better apportionment of the tests to the fields of investigation for which they are best adapted. The business of the philosopher would seem to be concerned especially with this last task. How remarkable and fruitful a revolution would take place in philosophy if philosophers would concern themselves, not in showing that in a certain remote region of thought a certain criterion of truth will not work (and thereby implying that it ought to be rejected completely), but rather in working out the most effective correlation between the various criteria and the types of problems to which they are to be applied!

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COEFFICIENTS OF DIAGNOSTIC VALUE

ONE of the reasons for the interest in the relation between performance in mental tests and specific or general abilities is the possibility of utilizing the former in the prediction of the latter. All that is needed to establish the possibility of such diagnosis is an unequivocal demonstration that the relation between performance in tests and the ability in question is close enough to permit diagnosis of such accuracy as the concrete situation requires. The necessity of measuring the observed relation between the variables is at once evident; and the most commonly used index of the closeness of the relation is the Bravais-Pearson coefficient of correlation (or some coefficient devised to give approximately the same result with the expenditure of less arithmetical effort).

Now, although it is clear that a close relation between performance in tests and the ability in question is associated with the possibility of accurate diagnosis, and although any increase in the closeness of the relationship is accompanied by an increase in the accuracy of diagnosis, it does not follow that an index which is used in describing the relationship can be taken *directly* as an index of the diagnostic value of mental tests. In fact, the coefficient of correlation, a widely used index of relationship, gives a decidedly misleading notion of diagnostic value. It is my purpose to show why the coefficient of correlation is misleading, and to suggest a coefficient which may be used with less confusion in stating the value of mental tests in practical situations.

The coefficient of correlation is a generalized statement of linear

regression. And "all the statistician means by regression is this: If all the organs A of a certain size have associated with them an array of B organs having a definite mean value, then this mean value changes with change of A. The distribution of the means of B arrays for given values of A, whether expressed by curve or table, is in its most general form the phenomenon which Mr. Galton has termed regression."[1] Thus, on the average, men who are x inches tall will have sons who are shorter than the sons of men who are $x+3$ inches in height. On the average, children who are eight years old will read a certain fifty words in less time than will children who are six years old. In each of these cases two variables are involved, the A and the B of Mr. Pearson's statement. In the first illustration, A is the height of fathers, B the height of sons; in the second illustration, A is the age of children, B is the time required to read a given fifty words.

Regression is said to be linear if equal increments of one variable are always associated with equal increments of the other. Concretely, if, on the average, the sons of men $x+3$ inches tall are 1 inch taller than are the sons of men x inches tall, no matter what the value of x may be, the regression is linear.

The regression coefficient in linear relations is the ratio of an increment in one variable to the associated increment in the other. In the example above, the regression coefficients are one third and three. If each variable is measured as a deviation from its mean, a general statement of the relation is the following:

$$x = b \cdot y$$

where b is the regression coefficient. Verbally this equation states: Individuals who differ from the average of variable Y by an amount y, on the average will differ from the average of variable X by an amount b times y, or x.

The scales in which the variables are measured are accidental facts in a majority of cases, facts which affect the relation between the variables not at all. In order to make all scales comparable, the variables may be measured in terms such that the standard deviation of each variable is 1. *When such scales are used, the regression coefficient becomes the coefficient of correlation.*

The coefficient of correlation, therefore, states the ratio of increments in one variable to corresponding increments in the other variable, when the regression in linear and when the standard deviations of the variables are unity (or equal). Thus the correlation coefficient is a natural statement of the regression phenomenon.

[1] K. Pearson, "On the Fundamental Conceptions of Biology," *Biometrika*, Vol. I., p. 323.

From the point of view of diagnosis, the correlation coefficient gives a means of determining preferred values in X for observed values in Y through the use of the regression equation. But the only reason for having preferred values at all is to increase the *accuracy* with which the prediction can be made; and the correlation coefficient is not a direct statement of the accuracy of prediction. It is simply a generalized statement of regression. Clearly the coefficient of correlation should not be used directly as an index of the accuracy with which diagnosis can be made, unless regression and accuracy of prediction can be shown to vary in a one to one relation.

Since the value of one variable is to be predicted from known values of another, it seems logical to measure the accuracy of diagnosis in terms of the amount of error that will be made in prediction by the linear regression equation.[2] Suppose for one individual the regression equation gives a value x_1; suppose that the true value is X_1. The error is $X_1 - x_1$. Call this error e_1. For a second individual, the error will be e_2, and for a third e_3. The sum of the squares of these errors divided by the total number of individuals is known as the mean square error of estimation. The square root of this quantity, the root mean square error of estimation, may be regarded as the standard deviation of the true values of the individuals around their predicted values. The root mean square error is sometimes called the *standard error of estimation;* it is an index of the dispersion of true values around predicted values, and might be taken directly as an index of the accuracy of diagnosis.

Again, as in the case of the coefficient of correlation, it is desirable to obtain a coefficient that is independent of the scales in which the variables are measured. Again, this independence is gained by expressing the measurements of the variables in such a way that the standard deviation of each variable is unity.

Fortunately, the individual errors need not be determined, and the scales need not be actually changed, for the generalized standard error may be obtained from a knowledge of r. The equation[3] is

$$\epsilon = (1 - r^2)^{\frac{1}{2}}$$

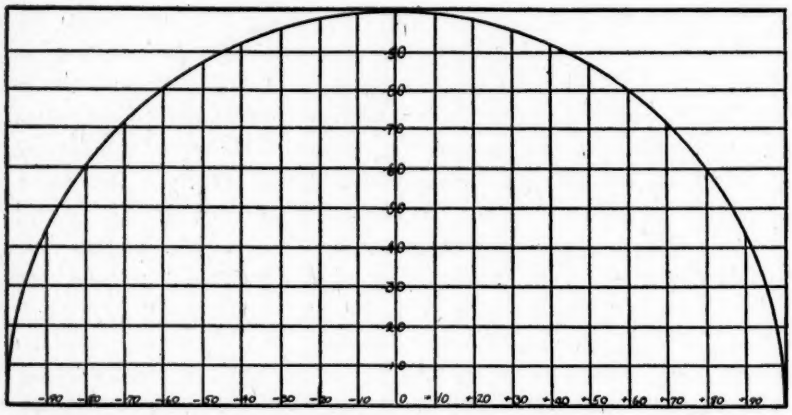
It is important to remember that the error coefficient which we shall use is the root mean square error of estimation when prediction is made from the linear regression equation, and when the standard deviation of each variable is unity.

[2] Discussion in this paper is limited to the case of linear regression. The significance of the linear equation in the general case is discussed by Mr. Yule in *Royal Society Proceedings*, Vol. 60, p. 477.

[3] Yule, *Introduction to the Theory of Statistics*, p. 177. The derivation of the formula is independent of an assumption of Gaussian distribution in the variables.

ϵ is the suggested index of the diagnostic value of mental (or other) tests. It expresses directly the magnitude of the error made in predicting the values of one variable from the values of another. Furthermore, it is easily derived from the coefficient of correlation.[4]

The meaning of ϵ is easily understood. If there is perfect linear relationship between the variables, ϵ equals 0; that is, there is no error in estimating one variable from another. If the two variables are independent, ϵ equals 1. The standard error of estimation is



The Relation between r and ϵ. Horizontal axis: coefficient of correlation. Vertical axis: error coefficient.

here equal to the standard deviation of the variable to be predicted; the accuracy of diagnosis will be only that which results from chance success. The maximum value of ϵ is 1; this is a logical limiting value, since it represents the error that is made in the wisest possible guesswork, that is, in giving all cases in the variable to be predicted the mean value. ϵ takes all values between 0 and 1; .5 means that the standard error made in predicting B from A is half as great as the error that would be made in the wisest guessing at the values of B; .25 means that the error is one quarter as great. These values, therefore, have a simple and easily understood meaning in the discussion of accuracy of diagnosis.

The coefficient of correlation is a misleading index of the diagnostic value of a test because it does not bear a one to one (or linear) relation to the standard error of estimation. The actual relation is as stated above. The graph of this relation is of considerable interest, and shows some striking facts. It will be seen that as r decreases from $+1$, or increases from -1, the standard error increases

[4] Dr. W. V. Bingham suggests that the fact that ϵ does not measure "reliability" in the psychological sense should be mentioned.

with extreme rapidity. Concretely, when r is equal to $\pm .86$, the standard error is half as great as it ever will be; when r equals $\pm .50$, the standard error is seven eighths its maximum size. *The amount of error made in predicting from the regression equation on the basis of a correlation of $+.44$ is nine tenths as great as the error made in guessing on the basis of no information whatever.*

Does all this mean that a correlation of $+.45$ indicates that a test has no diagnostic value? Not at all; for values are relative things that depend upon the accuracy of diagnosis that particular situations demand. In the most objective terms, ϵ indicates that the root mean square error of estimation in predictions on the basis of the regression equation and a correlation of $+.45$ is nine tenths as great as the error that would be made in predicting by the same method on the basis of a correlation of .00. The investigator must decide for himself whether such reduction in the error of estimation makes the test valuable in the particular situation in which the test is to be used. The point of the present paper is to emphasize the necessity, in judging diagnostic value, of recognizing that the error of estimation is not reduced directly as the correlation is increased, of recognizing, for example, that a correlation of $+.60$ reduces the error of estimation only two tenths. I believe for these reasons that r is misleading, and that when diagnostic value is discussed, ϵ might be used as an index with advantage.

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REVIEWS AND ABSTRACTS OF LITERATURE

Egotism in German Philosophy. GEORGE SANTAYANA. London and Toronto: J. M. Dent and Sons, Ltd.; New York: Chas. Scribner's Sons. Pp. 171.

To the many hard things that are nowadays being said against Germany Mr. Santayana adds the accusation of Protestantism. That the Germans have long since adopted Luther as a patron saint and claimed him as exclusively their own, is well known. But this has commonly been cited as evidence of their tendency to nationalize and appropriate the common European heritage. Most of the critics of Germany have been themselves Protestants who were unwilling that their faith should be identified with the creed of a militant nationalism. Mr. Santayana has no such scruple. Their philosophy, he says, "is Protestant theology rationalized" (p. 22); and it is clear that so saying he means to disparage both German philosophy and Prot-

estant theology. But the argument is obscure, and scarcely convincing. Thus we are told that in the philosophy of Fichte and Hegel "the principal figures are not individuals, like the Creator, the Redeemer, and one's own soul, but nations and institutions." Now it is commonly supposed that it is Catholicism and not Protestantism which is institutional. In any case Mr Santayana himself says that Protestantism "is reformation essentially, in that every individual must reinterpret the Bible and the practises of the church in his own spirit" (p. 26). Again, "the Protestant precept to search the Scriptures, and the sense that every man must settle the highest questions for himself, have contributed to the zeal with which science and scholarship have been pursued in Germany" (pp. 28–29). This would seem to contradict not only the institutionalism which the author ascribes to Protestant Germany, but also the romantic illogicality, the pedantry, and above all the excessive docility or submissiveness which the German is supposed to show toward authority. Finally, Mr. Santayana presents us with two quite opposite versions of the spirit of the Protestant religion. On the one hand, Protestantism is a revival of primitive Christianity, a sort of religious experimentalism, in which the believer must renew the teaching and the practise of the church "in the light of his personal religious experience" (p. 26). But on the other hand, we are told that "it is of the essence of Protestantism and of German philosophy that religion should gradually drop its supernatural personages and comforting private hopes and be absorbed in the duty of living manfully and conscientiously the conventional life of this world. Positive religion, in fact, disappears . . . and there remains only a consecrated worldliness that is deliberate and imposed as a duty" (p. 23). The former of these two interpretations possesses a broad historical justification; the second would appear to reflect the author's first-hand acquaintance with New England Unitarianism. But in any case so ambiguous a version of "the Protestant heritage" can scarcely be expected to afford the key to the interpretation of a national culture.

"The soul of German philosophy," according to Mr. Santayana, is "egotism—subjectivity in thought and wilfulness in morals" (p. 6). It focusses on the media of knowledge, on its "veils and lenses," and thereby contradicts the very essence of knowledge, which is to report external objects. Thus "Hegel was a solemn sophist: he made discourse the key to reality" (p. 84). Transcendentalism is "this method of looking for reality within one's own breast" (p. 32); and of loving nature and history promiscuously as creatures of one's own making. Morally and spiritually this philosophy is a return to that primitive heathenism which "stands bravely before a painted world, covets some bauble, and defies

death;" to " the religion of the will, the faith which life has in itself because it is life, and in its aims because it is pursuing them " (p. 149). Such is the essential significance of Schopenhauer, Wagner, Nietzsche, and even Goethe.

Mr. Santayana's characterization of the German national genius is witty, apt, and irresistibly quotable. In the judgment of the present writer it is substantially true; as true, perhaps, as any such sweeping generalization can hope to be. Nevertheless, there are omissions and exaggerations in the account that are so obvious as to suggest caricature rather than criticism. Thus there is a sense in which it is true that transcendentalism is "looking for reality in one's own breast;" but this is by no means the whole of the truth, and it could be contended with much plausibility that it is the least important part of the truth. The dominant motive in Kant was not subjectivism, but rationalism. He was primarily concerned to reestablish the claims of exact science, to demonstrate the logical and mathematical structure of nature. Furthermore, Kant made important contributions to logic itself through his method of searching for presuppositions and postulates. It is true that he believed that the claims of exact science could be established only by regarding nature as phenomenal, and that his logic was confused by an admixture of grammar and psychology. But the fact remains that it is chiefly owing to Kant and his followers that philosophy has been diverted from sensationalism and the Platonic tradition kept alive. So much even realists must acknowledge. That the proponents of "creative intelligence" owe him an even greater debt need scarcely be remarked.

Mr. Santayana's account is similarly inadequate on the moral and spiritual side. Though he makes a brave effort, he is, in my judgment, quite unable to deduce from his formula of romantic heathenism such glaring traits of the German as his capacity for patient research and for organization, his pedantry, his dull sentimentality, his honesty, and law-abidingness. Heathenism, we are told, obeys " the inward and vital imperative which the bull obeys, when trusting absolutely in his own strength, rage, and courage, he follows a little red flag and his destiny this way and that way " (p. 153). But the fact is that the most notable thing about the German is his discipline. The best that the author can do to reconcile these contradictory opposites is to say that the real egotist will take his own subjective code so seriously that " he would feel it to be derogatory to his dignity, and contrary to his settled purpose to cheat at the game he has instituted " (p. 163). But this is not convincing. It is an ingenious paradox; but it leaves the reader with the feeling that it would have been better to admit that

when it comes to phenomena so complex as races and nations, some things must be left unexplained. For this reason the book before us suffers in comparison with Professor Dewey's *German Philosophy and Politics*. The latter is less literary and less entertaining; but perhaps for that very reason it conveys the impression of being more restrained and judicial. Furthermore, Professor Dewey is more skilful in his selection of a formula, for one can deduce at least twice as much from dualism as from egotism.

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Religious Values and Intellectual Consistency. EDWARD HARTMAN REISNER. New York: The Science Press. 1915. Pp. 59.

What do we mean nowadays by the words God and Religion? and can we really mean much of anything and yet be intellectually consistent? These are the questions that Dr. Reisner tries to answer in this book, which was his thesis for the degree of Doctor of Philosophy.

The author defines religion as an emotional attitude towards the whole of one's experience, and as concerned with judgments of value. Therefore it demands intellectual consistency. The concept of God has always done duty as the guardian of values, and so an historical survey of the God concept may throw light on some of the difficulties of religious philosophy.

The author then gives an account of the growth of Christian dogma and its disintegration, due to the growth of science and to the fact that the critical philosophy of the eighteenth century undermined the intellectual foundations of classical Christianity. The followers of Kant tried to rebuild religion on a basis of absolute idealism. Dr. Reisner gives the usual criticism of Fichte, Hegel, and Royce, and finds that idealism and its absolute have not succeeded in finding a metaphysical basis for the concept of God which is intellectually consistent.

The author then attempts to analyze the concept of God as it exists to-day in the minds of educated people. He believes that any attempt to find a God through philosophy will fail—"God must exist to be discovered and described." He finds that the God concept of the present time is a name representative of certain values, and that "the conception of God as a Being represents an historical phase of religion that modern scientific knowledge finds no place for." Religion is a natural, psychological product and must be founded on an empirical basis. There are religious experiences that are genuine and real, and for the religious man who is also scientific, God might well be the name or symbol for these experiences. "God is the name for the reality of religious experiences; the religious

experiences are the reality of God. God and religion are synonymous, and both stand for the reality of a realized or realizable experience."

The constructive part of this book is so interesting and alive that one regrets that so much space was given to the rather formal and perfunctory refutation of absolute idealism, which nowadays is hardly enough of a bug-a-boo to demand such a thoroughgoing exposure. The kind of discussion that centers around the author's ideas of religious values and religious experiences is the kind of discussion that people of to-day, vitally interested in religion as a fact of life, welcome and find interesting, although many may not agree with the author's conclusions.

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JOURNALS AND NEW BOOKS

REVUE PHILOSOPHIQUE. February, 1917. *Le langage* (pp. 105–131): DR. BARAT. – The author, following P. Marie, insists that aphasias involve a certain intellectual trouble. "That which is specific in aphasic troubles, is the injury, not of intellectual activity as applied to the comprehension of signs, but of the materials and tools of logical thought. These tools and materials are the automatic associations which intervene in the coordination of phonetic movements, in the analysis and interpretation of the sensation, in the evocation and identification of images." *La fonction et l'idéal de la géométrie* (pp. 132–170): PAUL DUPONT. – Chief conclusions: "the order of first propositions of geometry is not necessary and that of the classic treatises does not appear best. . . ." Classical geometry fails in that " it appeals to the three notions of plane, point, and straight line without defining them completely and employs confusedly unconscious postulates"; "it introduces the postulate of Euclid under censurable forms." These errors are to be redressed by taking as primitively given only the notions of the point (element), and the variable coupling of two points (relation); by reforming the theory of parallels, and by "giving to the postulate of Euclid a form relative to the primitive elements of the point and coupling drawn directly from nature." *L'idéalisme positif* (pp. 171–189) (to be continued): DR. GRASSET. – Positive idealism, holding the idea or the human psychic fact as the base of all knowledge, and accepting the positive method, is grounded on human biology. The psychic fact is a cerebral cortical fact, and, therefore, somatic and physiological. "Superior psychic acts" are voluntary and conscious: "inferior psychic acts" are automatic and

unconscious. Complex psychical processes (*e. g.*, emotion) comprise two orders of elements, psychic elements, and non-psychic elements (*e. g.*, vaso-motor and circulatory phenomena). But both are physiological. The writer denies an intention to identify the two elements and repudiates physico-chemical monism, but insists that the "study of the cerebral part of the psychical function can be attached to human physiology and biology, though not to general biology. *Revue Critique.* Enzo Bonaventura, *Ricerche sperimentale sulle illusioni dell' introspezione:* FR. PAULHAN. *Analyses et comptes rendus.* Hugo Münsterberg, *Psychology and Industrial Efficiency:* DR. JEAN PHILLIPE. Giovanni Vidari, *Elementi di pedagogia: 1. I Dati della pedagogia:* J. PÉRÈS. *Revue des périodiques.*

JOURNAL OF EXPERIMENTAL PSYCHOLOGY. April, 1917. *Some Tests on Memorizing of Musical Themes* (pp. 93–99): KATE GORDON. – Piano music was played by the experimenter at a fixed rate of speed determined by the ticking of a clock. The subject was required to sing, hum, or whistle as much as he could recall. The selection was repeated until the subject could render it perfectly twice. Nonsense syllables were also learned and tests were made where the subject learned reversed musical selections. Among other results were the following: for the musical group musical selections were easier to learn than nonsense syllables and for the unmusical group music was harder than syllables. The difference in ease of memorizing between nonsense syllables and music was less marked than between significant material and nonsense material. It is possible that a test of musical appreciation might be constructed which should hinge upon the difference in memorizing a significant and non-significant musical selection. *Quantitative Tonal Stimuli without Qualitative Change* (pp. 100–105): HAROLD A. RICHMOND. – A method is explained by which it is possible to produce an abrupt but smooth change in the intensity of a simple tone of high vibration frequency. *The Focal Vibrator* (pp. 106–113): A. P. WEISS. – Apparatus is described which is essentially a system of lenses so related to each other that a visual stimulus may be projected on a ground-glass screen. The apparatus controls several stimulus conditions. *Hipp Chronoscope without Springs* (pp. 114–116): HOWARD C. WARREN and PRENTICE REEVES. – An arrangement is described where the adjustments of Dunlap are used and the springs counterbalanced by an added weight. *Systolic Blood Pressure Symptoms of Deception* (pp. 117–163): WILLIAM M. MARSTON. – The problem was to investigate changes in blood pressure resulting from an effort to hide the truth. Among other conclusions are: The behavior of the blood pressure does not act as the least indicator of the objective validity of the story

told by any witness, but it constitutes a practically infallible test of the *consciousness of an attitude of deception.*

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Sinclair, May. A Defence of Idealism. New York: The Macmillan Company. 1917. Pp. xvii + 355. $2.00.

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NOTES AND NEWS

Letter from Professor Alexander

To the Editors of The Journal of Philosophy, Psychology, and Scientific Methods:

Professor Creighton's suggestion, in a recent number of this Journal, that the programmes of the American Philosophical Association be made a matter of constructive discussion seems to me admirable. I should like to add an expression of a hope that the field of the discussion be enlarged to comprehend not only the programmes, but the mutual relationships of all the associations in the country devoted to philosophy. Members of the Western Philosophical Association are by no means satisfied with the attitude of the American Association toward their plan of amalgamation, and hope for a reconsideration of that plan or for the suggestion of some other, perhaps more generally agreeable. In a country territorially so vast as is the United States it is obvious that regional associations must grow in number with the passage of time; indeed, all North

America ought to be the continental field for a single group of related associations, all gathering strength from their community of interest, and, if possible, of organization. It might be possible to have these groups organized for separate annual meetings and to have a triennial meeting of all, or of large delegations from all, at Washington, or some other accessible point. If at the same time related sciences, especially the whole group of the sciences of man (psychology, anthropology, sociology, political science, and economy), could meet along with their mother, philosophy, the triennial meeting could assume the proportions of an important congress, of much significance for the learning of the continent. Surely there is no question that one of the great problems of the civilization of the continent is the common understanding of its ideals or that the bond of such understanding should be the universities. And what science is in better position to prepare the way than is philosophy?

Very truly yours,

H. B. ALEXANDER.

UNIVERSITY OF NEBRASKA,
September, 1917.

THE appointments to the staff of the newly organized department of psychology at the University of Minnesota, of which Professor Robert M. Yerkes is to be chairman, as so far announced are as follows: Associate professor, Herbert H. Woodrow; assistant professors, Henry T. Moore and Joseph Peterson; instructor, K. S. Lashley.

THE Section of Anthropology and Psychology met in conjunction with the American Ethnological Society on October 22. A "Statistical Study of the Growth of Children" by Professor Franz Boas, Mr. Leslie Spier, and Mr. Louis R. Sullivan was presented.

DR. ETHEL BOWMAN, Wellesley, 1900, Ph.D., Clark University, 1917, has been appointed associate professor of psychology at Goucher College.

THE next annual meeting of the American Psychological Association will be held at Pittsburgh from December 27 to 29, 1917, instead of at Ann Arbor.

PROFESSOR E. H. LINDLEY, of the University of Indiana, has been elected president of the University of Idaho.